

# **On the gift of eternal youth<sup>\*</sup>**

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## **Note**

The contrast between Weber's aphorism of eternal youth and Merton's concern with the adolescent stage of sociology was too striking to stop us from working on it. The result, this paper, aims rather to make a point in Weber's favor than to be accurate. A concern with accuracy would loose up the argument, and, moreover, make it difficult to confine it to its limitations of space and time. Perhaps two main flows should be noticed. In the opposition we make between the American and the European tradition, in part three, there was no place for symbolic interactionism, in the American side, and the more purely French sociology, Durkheim himself, in the European. Perhaps a few gimmicks could fit them somewhere, but this was not done. The other flaw is the lack of any reference to Weber's ideal types, that come so close, in the texts, to what we discuss here. We preferred, however, to stay out of the intricacies of the problem.

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## On the gift of eternal youth

"Moreover, there are sciences to which eternal youth is granted, and the historical disciplines are among them - all those which the eternally onward flowing stream of culture perpetually brings new problems" (Max Weber, 'Objectivity' in Social Sciences and Social Policy).<sup>1</sup>

### I

Max Weber was well aware, at the turn of the century, that the social sciences did not have the characteristics of a mature discipline - and he seemed to like it. But forty years afterwards, this same youth did not seem to make Prof. Robert K. Merton as happy. To him, immaturity of social sciences was a good reason to keep them restricted to middle-range theorizing, and Merton indeed regretted the absence of Giants over whose mighty shoulders one could grasp the longed coming of age of the social disciplines<sup>2</sup>.

The embarrassing inability of the social sciences to behave properly, as other mature sciences do, is, perhaps, at the basis of all the methodological and epistemological argument in the field. Few disagree that this inability really exists, the questions being, first, whether this is a good or a bad thing, and, second, what kind of consequences should be drawn from it. Do we have to assume, with Merton, that social sciences<sup>3</sup> are not that old, actually, when measured in terms of hours of research, and thus it is a normal fact that they do not have either the stability nor the explanatory strength of the natural sciences? Or shall we agree that the eternal youth of the social sciences is not a question of age, nor of accumulation of findings, but, as Weber puts it, a happy consequence of their proximity with the "eternally onward flowing stream of culture"?

Both answers are far from satisfactory. We know that sociology is very different from the natural sciences, with little theoretical integration, weak or existing deductive systems, poor degree of

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<sup>1</sup>Glencoe, Ill.: The Free Press, 1949. Translation from Die "Objektivität" sozialwissenschaftlicher und sozialpolitischer Erkenntnis, by E. Shils and H. F. Finch.

<sup>2</sup>*Social Theory and Social Structure*, introduction. On the giant's aphorism, see below.

<sup>3</sup>Merton talks about "sociology", while Weber uses terms like social sciences, social politics, social economics, historical sciences, sciences of culture, and so on. Are they really referring to the same thing? we believe they are, this thing being, to start with, a residual category or social science that remains after the fields of economics, historiography, psychology, linguistics and law are taken away. We would include political science and anthropology as also covered by the terms "sociology" and "social sciences", which we will use indifferently in the following.

confirmation of its propositions, conceptual chaos and little cumulativeness of research. The natural sciences, and more specifically physics, give the pattern of how it ought to be, and, according to Merton, it will be sometime in the future. The characteristics of what is a mature science (the mirror into which sociology looks so poor) are derived both from the experience of the natural disciplines and from the logic and epistemology that are derived from them. If the structure of scientific statements is unique and single, this pattern, that is imposed in a disciplinary manner over the social sciences, seems to be unavoidable. To give up this postulate of the unity of scientific knowledge would mean, as it has meant to many students, to look for non-empirical forms of knowledge that are incompatible with the accepted and successful canons of intersubjectivity, verification and refutability that are common to all sciences.

However, in spite of the strong backing offered by the natural sciences, the point of view that Merton represents is not without difficulties.

The first difficulty is that sociology did not seem to become more mature in the period going from Weber's statement on eternal youth and the publication of Merton's ideas on the middle-range theories<sup>4</sup>. And the successive re-editions of his *Social Theory and Social Structure*, always relevant and up-to-date - notwithstanding the maze of research efforts in the social sciences in the post-war period - seems to indicate that perhaps the eternal youth aphorism was not as far-fetched as it may seem<sup>5</sup>.

To this bit of empirical falsification it is possible to add that Merton never shows how the present tendencies of development of sociology are leading to the emergence of the characteristics that it "ought" to have. The same could be said about the majority of discussions in the field, including Kuhn, who considers social sciences as "pre-paradigmatic", according to the conception derived from the history of natural sciences.<sup>6</sup>

Illuminating as the analogy with the grown-up disciplines might be, the danger always exists of imposing a model that has little to do with the real practice and tendencies of the younger one. To derive the characteristics of a given science from a normative model is, as G. G. Granger puts it, to make "l'herméneutique d'une mythologie"<sup>7</sup>. He does not deny that the actual practice of a science "enveloppe-t-elle bien à chaque étape un idéal de la connaissance", but, he concludes, "il importe de ne pas confondre cet idéal, qui est partie intégrante de la pensée scientifique comme fait, avec une norme

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<sup>4</sup>It appeared first in a paper on "the position of sociological theory", *American Sociological Review*, 1948, 13, and was afterwards included in the introduction of *Social Theory and Social Structure*. Middle-range theories, the giant's aphorism and Whitehead's warning on the need of forgetting the founders are the three basis of a picture of sociology brought forth by Merton that became dominant, and are expressed in his classical introduction to this book.

<sup>5</sup>Merton suggests in a footnote to the 1957 edition of his book that some convergence is occurring in sociology, but rather at the level of general orientations than at the level of theory formation. And he is optimistic enough to consider the former as an anticipation of the latter: "But manifestly, not everything can happen at once: the gain in convergence is real even though it is partial rather than complete" (p. 9n).

<sup>6</sup>Kuhn, T. S.: *The Structure of Scientific Revolutions*, Chicago: Univ. of Chicago Press, 1962.

<sup>7</sup>Granger, Gilles-Gaston: *Pensée Formelle et Sciences de L'Homme*, Paris, Aubier, 1960. Cf. also "Événement et Structure dans les Sciences de l'Homme", *Cahiers de l'ISEA, Dialogues*, 1, 1967.

universelle et prédéterminée. La science existe en fait; la difficulté préliminaire la plus paradoxale de l'épistémologie est de la saisir comme telle, sans lui substituer une image hypostasiée." .

Our suspicion is that this preliminary difficulty is not being considered and overcome; worse, the "mythology" that is being used as a model for sociology is not even derived from an ideal of the social sciences itself, but from another field. The consequence has been the attempt to impose a straight-jacket upon sociology as the only "normal" thing to do, the other forms of social analysis falling outside the frame of acceptability.

It will be impossible, in this paper, to demonstrate the accuracy of this suspicion, let alone to give a correct answer to the problem - if there is such an answer. What we are trying to do in this first part of the paper is to put together some hints that could strengthen our suspicion. What we shall do next will be to rise a second suspicion we have: that perhaps sociology had, after all, been granted the gift of eternal youth, and that this does not mean any radical departure from the canons of sound epistemology.

Let us conclude this part with a more careful look at the giant's aphorism.<sup>8</sup> It is remarkable that the need for a giant sociologist is indicated in the very same passage where the necessity of forgetting our forefathers is also stressed. The paradox of at the same time striving for the emergence of giants and suggesting the need of working at the middle-range level, forgetting the few giants we have, is all too evident, Let us quote Merton:

We sociologists of today may be only intellectual pygmies but, unlike the overly modest Newton, we are not pygmies on the shoulders of giants. The accumulative tradition is still so slight that the shoulders of the giants of sociological science do not provide a very solid base on which to stand. Whitehead's apothegm, affixed to the masthead of this introduction, is therefore all the more binding on sociology than on those physical sciences which have a larger measure of selectively accumulative advance: 'a science which hesitates to forget its founders is lost' <sup>9</sup>.

The conclusion seems to be that, the more we need giants, the more we have to forget about them...

Merton's message, as a warning against excessive speculation and personalization of sociology, at a time when new techniques of social research were being developed and needed implementation, was and still is, no doubt, a valuable and relevant one. But it is not so sure that he draws a correct picture of how science really develops, nor that his warning has to be taken literally in all circumstances.

The idea of scientific Giants is not a simple figure of speech. It is well accepted today that science is organized around paradigms, and these paradigms are developed around the general

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<sup>8</sup>The giant's aphorism seems to have a long and relevant history both for Professor Merton and for science in general. For both, see Merton, R. K., *On the Shoulders of Giants, A Shandean Postscript*, New York, Free Press, 1965.

<sup>9</sup>Op. cit., p. 5.

theoretical frame and style of work of a man. As Kuhn clearly shows<sup>10</sup>, different paradigms imply differences in style, criteria of truth, norms of "adequate" scientific procedures, etc. New paradigms are inaugurated by outstanding scientists that are able, due to intellectual, psychological and sociological characteristics, to profit from the "selectively accumulative" advance of research that leads to exhaustion of the previous paradigm. The conclusions we take from this are that, first, science develops by big leaps, second, these leaps are the outcome of personalized work, and, third, this is not a purely "scientific" affair that has to do only with the intrinsic needs and potentialities of a discipline as an abstract body of knowledge.

What leads to the emergence of scientific giants is a question that might not have a simple answer. What seems to be clear, however, is that the conditions put forth by Merton are not the best ones for this purpose.

The main weakness of the argument is that it does not indicate how, exactly, the accumulation of research leads to the emergence of scientific giants, and the consolidation of paradigms<sup>11</sup>. "Selectively cumulative advance" implies the existence of a common framework that is "forgotten" insofar as it is taken for granted. The search for this common basis is exactly the characteristics of a modern, or young discipline, as the search for identity is the characteristics of adolescence. If this is so, it is not only pointless but also dysfunctional to urge sociology to forget its identity problems and behave properly, as an unsophisticated parent would do with his over-anxious son. In short, it is still too early for sociology to forget its founders, even if we accept Merton's premises that some day they will fall, not exactly into oblivion, but into the area of the unconscious. An excessive emphasis on middle-range theorizing, and a premature repression of more speculative activities can mean the proliferation of piecemeal research that have not only little relevance, but, worse, non-cumulativity. Because it is indeed very doubtful that the simple quantitative accumulation of irrelevant research really paves the way for the arrival of the giants.

In spite of its weakness, Merton's argument is important insofar as it is supported by the example of the success of other sciences and, mostly, because it indicates a way out from the anxieties of excessive speculation. These anxieties will be discussed later on. But let us, first, examine Weber's reasoning on the eternal youth.

## II

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<sup>10</sup>Op. cit., specially pages 93 and following

<sup>11</sup>It is said that about ninety percent of all scientists that ever existed are living today. In terms of research effort, the disproportion between the output of contemporary scientists and their predecessors must be even bigger. Although the share of social sciences in this bulk of work is very small, it would not be far-fetched to suppose that the amount of time, resources and intelligence dedicated to the social sciences in the post-war period (that means, since Merton's text was published) is much bigger than all the effort dedicated to physics before Newton. And we are still without our Kepler! As a matter of fact, we have no criteria for saying when a "satisfactory" level of convergence is obtained, let alone of how many research-hours are necessary to have a science ready for maturity or for the emergence of a Giant of a given size.

A short summary of Weber's point of view, which we hope is not a too big a distortion of what he really meant, could run as follows.

Weber characterizes the object of the social science as *historical*, and this according to two different meanings. Social facts are historical because they occur in space and time, they have a present, a past and a future, and are subject to change. In this sense social science is very different from natural sciences, which deal with phenomena that are general and invariant in space and time. They are also historical in the sense that they are meaningful facts, worth knowing only as a function of value ideas ("Besiegung auf Wertideen")<sup>12</sup> and the value ideas are also subject to historical change. This means that the "same" object, which is by itself inarticulate and meaningless, acquires different shape and color according to the changing value ideas, and has to be studied again and again according to different points of view.

The consequences Weber derives from this characterization of the scientific object of social disciplines is clearly expressed by the stages he thinks social explanation must follow. There are three, or perhaps four stages<sup>13</sup>:

1. The first is the knowledge of general laws, or of establishing a general theory that could analyze and reduce to a few simple factors all the causal nexus of human interaction. Weber is very skeptical about the real possibilities of developing such a theory, but he grants that it is not logically impossible - although he does believe that it would never be very relevant in any case.
2. The second stage is the analysis of a given specific combination of the previous factors in a meaningful way. In Weber's words: "The analysis of the historically given individual configuration of those 'factors' and their significant concrete interaction, conditioned by their historical context and specially by *rendering intelligible* the basis and type of this significance would be the next task to be achieved"<sup>14</sup>.
3. The third stage is the causal explanation, in historical terms: to single out the specific causes of the given specific phenomena we are studying.
4. The last stage that can be conceived is the prediction of "possible future constellations".

It is interesting to notice the little attention Weber gives to questions of prediction, in contrast

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<sup>12</sup>The French translation by Julien Freund, *Essais sur la Théorie de la Science*, Paris, Plon, 1965) gives as "value ideas" what Shils' translation calls "evaluative ideas". Other nuances of meaning can be found, for instance when the "stages", below, are "tasks" in Shils' translation. The reference here is p. 159 of the French text, and 81 of the English.

<sup>13</sup>ibid, p.157 (French), p.79 (English).

<sup>14</sup>ibid p.158 (French) and 79-80 (English)

with the tendency today that takes the possibilities of foresight as the very criterion of scientificity.<sup>15</sup> The radical opposition that he introduces between general and "legal" explanation, on the one hand, and specific and historical explanation on the other, is obviously a consequence of his inability to perceive the possibility of studying general tendencies and processes that would not be dissolved into a "calculus" of society nor fall in the contradiction of "singular causality" (see below). Weber was obviously mistaken when he considered only the possibility of a general sociology at the psychologic level. Regarding the problem of historical causality, there is little doubt, after Popper<sup>16</sup>, that the so-called "historical" explanations simply keep the general hypothesis they apply in an implicit form.

The second stage of analysis gives rise to more difficult questions. It is from here that all the "verstehen" approach emerges, and it is also from here that the aphorism of eternal youth is also taken. Our suggestion is that these two consequences are not necessarily connected, although they usually appear empirically together, as with Weber. Before examining this point in more detail we must, first, come back to the question of the anxieties that a more unstable approach to social sciences is bound to create in some circumstances.

### III

What differentiates an American from an European sociologist, suggests Merton<sup>17</sup>, is that, while the American is concerned with the truth of his statement, regardless of its relevance, for an European it is the relevance that matters most, the truth not being as important.

This graphic "boutade" points to the drastically different traditions between the European and the North American social thought and the kind of social sciences that came out from them. It is not only a difference on intellectual orientations, but also on the role-sets of the social scientists in the two regions. Although an accurate picture of these differences would demand a research work on its own, it is possible to give here the main traits that makes them so clear-cut.

L. .L. and Jessie Bernard, in the Introduction to their book on the *Origins of American Sociology*<sup>18</sup> indicate the existence of two streams of social thought that have their origins in the nineteenth century:

"One of these, the liberal democratic tradition, developed primarily in France and England. It stressed the importance of reason, of natural laws, of science, of the individual and it minimized the state. The other, in large measure a German product, was authoritarian in character. It

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<sup>15</sup>Cf. Toulmin, S. *Foresight and Understanding*, New York. Harper, 1963.

<sup>16</sup>Popper, K, *The Poverty of Historicism*, New York, Harper, 1964, specially pages 143-147.

<sup>17</sup>In the introduction to the section on sociology of knowledge of *Social Theory and Social Structure* (p.440).

<sup>18</sup>New York, Russel & Russel, 1965.

stressed the importance of the culture, the nation, the folk, the race, the state."<sup>19</sup>

The liberal tradition, according to the Bernard's, is at the basis of the Social Science Movement, the main stream from which the North American sociology is a development.

The social role of the American social scientist, at its beginnings, can be seen through the purposes of the American Social Sciences Association, defined in 1866 to be "to aid the development of Social Science, and to guide the public mind to the best practical means of promoting the Amendment of Laws, the Advancement of Education, the Prevention and Repression of Crime, the Reformation of Criminals, and the Progress of public Morality, the adoption of Sanitary Regulations and the diffusion of sound principles on the Questions of Economy, Trade and Finance".<sup>20</sup>

Two roles come out from this picture: one, vague, of "developing Social Science" and another much more specific and detailed, that amount to the functions of social work. Only the first was really academic, but only could get some legitimacy insofar as the ideal of a global Social Science was substituted by the development of specific and partial social disciplines. The difficulty with sociology was exactly that it was not easy to single out its specific scientific dominion: it was a kind of residual discipline, after the detachment of economics, political science, education, public health, etc. And the Bernard's indicate how Sociology, "the most immediate successor of Social Science", was able to survive mainly for being "for the most part content to serve humbly by developing the neglected and minor aspects of the social sciences in the college curricula". And the expansion of the North American college system was big enough, at that time, to absorb these humble social scientists.<sup>21</sup>

In short, the only legitimate social role for a sociologist, outside the academic milieu, was in the fields of social welfare and charity, a kind of role that was taken over by specialists. The sociologist had to stay confined to his university, then, trying to develop a discipline that could be as scientific and respectable as the natural sciences or the oldest fields of social sciences, and at the same time avoiding to touch questions of politics, social change, characteristics of the State, etc. All this was incompatible both with their cultural inheritance, that took these questions for granted, and with their role in society that did not expect these functions from them.

The history of European, or German social thought, is quite different. The early formation of the German state, for one thing, could not possibly keep the problems of state and politics, and historical

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<sup>19</sup>Of course, all these geographical boundaries are approximations. The inclusion of France in the Anglo-Saxon tradition is only partially correct. The Hegelian influence was always present in this country, and, after the War, the leading orientation in French sociology, as expressed through the *Cahiers Internationaux de Sociologie* (under the direction of G. Gurvitch) showed the strong and often simultaneous influence of Marxism, Phenomenology and Psychoanalysis, all German products, besides Hegel himself. It is possible to say that the German-French tradition spread through the underdeveloped world much more widely than the Anglo-Saxon one, which became preponderant in Northern Europe. This was more or less the picture until the end of the fifties, when it began to change.

<sup>20</sup>Quoted by L. L. and Jessie Bernard, op. it., p. 562.

<sup>21</sup>ibid., p. 594-5.



change, outside the sight of the social scientist. Besides, the relevance of the social role of a German Professor was out of proportion with the little relevance it had in the United States. Social sciences inherited the tradition and the prestige of the German philosophers, that were perceived and perceived themselves as the very personification of the best of their cultures. And even Weber, who strongly insisted upon the difference between the normative and the scientific aspects of social thought, never neglected political participation, being reported to have said, at the end of his life, that he was not born for science, but for the pen and the tribune of the orators.<sup>22</sup>

If this picture is correct, the social role of the European social scientist was much more demanding and pretentious than the role of his American colleague. Besides his academic responsibilities, which were high in an environment of strong philosophical influence, he had to account for a public role. He had to attend expectations of answers to the political and social problems of his time, and could not overlook the fact that concepts like state, values, culture, nation, etc., were at stake and had to be accounted for. The kind of answers he had to give, both in his academic and public roles, created a range of concerns, and a kind of style<sup>23</sup> that could not be easily transplanted to a context where the social definition of the place of the social scientist was so different. No wonder that this speculative mode, this continuous changing references to changing processes, and with changing concepts, was bound to create anxiety and rejection from the American social scientist. Little wonder, on the other hand, that the excessive modesty and precision that Merton attributed to sociology created the same kind of rejection and anxiety in the other side of the Atlantic.

#### IV

Sociology of knowledge does not substitute epistemology, and the differences in the role of the social scientist in the two contexts does not tell us about the quality of the social sciences they produce. But it is very difficult to say, in general, which orientation had better outcomes, when the very definition of a "valuable" outcome depends on the values that are implied on each orientation, or paradigm. Only when a consensus is reached on what is to be explained, and on what is a good explanation, do comparisons between different orientations become meaningful<sup>24</sup>

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<sup>22</sup>Quoted by Weinreich, M., *Max Weber, l'Homme et le Savant*, Paris: J. Vrin, 1938. Bendix gives also a good picture of Weber's political nostalgia, mainly with the help of references taken from the work of Marianne Weber. Cf. Bendix, R., *Max Weber, An Intellectual Portrait*, London: Methuen & Co Ltd, 1966, chapter 1.

<sup>23</sup>Whether this style is authoritarian or not is an open question. No doubt that the German idealism is at the roots of European nationalism, but it is also at the roots of European social democracy, whose anti-totalitarian role in Europe cannot be disregarded (we are thinking, of course, in Marx). While it is possible to consider Hegel an authoritarian, the same can hardly be said from Marx or Weber, who Bendix considers as being, by his works, "an analysis and defense of Western civilization". That Marxism lead to authoritarian forms of political participation is a question that is better discussed in sociological terms, as for instance in Michels, than in ideological or theoretical ones.

<sup>24</sup>A possible way out would be to measure the anxiety that the disproportion between the goal and the actual achievements produces for the two orientations. But this anxiety depends on factors like the level of aspiration, in the first place, and also on the possibilities of transforming the initial frustration into something "normal" and "acceptable". This could be achieved, for instance, in a science that is fully professionalized and self-sufficient, in such a way that the only clients and judges of the quality of one's work is one's colleagues. It would be very easy, in a situation like that,

I believe that Weber and Merton point, actually, to different Paradigms of social science, that only today are finding a common ground. These different paradigms had, if not a common origin, at least a contact between their European founders in the eighteen and nineteen century. They were driven apart by differences on emphasis that are related to differences in social context, as we indicated before. These differences in emphasis became, in many instances, divergence on basic assumptions that lead to misunderstandings and lack of communication. We shall present a short discussion of these different emphases, and turn afterwards to the differences in assumptions.

It is possible to think for the sake of this discussion, that any science, at a given moment, is a function of two variables: one is the adequacy of the conceptual system to grasp the problems that are relevant from an extra-scientific standpoint. The other is the accuracy by which this system is defined and the relationships between the different concepts are established. The more a science is mature, the more it is possible to have both accuracy and relevance. But a young discipline has to compromise. We can further suppose that these two variables are commensurable, so that it is possible to say that an "x" amount of A (accuracy) is equivalent to a "y" amount of R (relevance). Moreover, it would not be unreasonable to suppose that, at any given level of technological and conceptual development, the relation between our two variables is constant, that is,  $A + R = K$ . If the maximum of K is an arbitrary 10, as in figure 1 (which would correspond to a full-fledged science). Figure 2 shows the possibilities of types of science with a level of  $K = 8$ , and figure 3 shows the possible types with  $K = 5$ . By definition, there is no case with A or R equal to zero.

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to develop standards of evaluation that are so permissive that would reduce the possibilities of anxieties to a minimum.

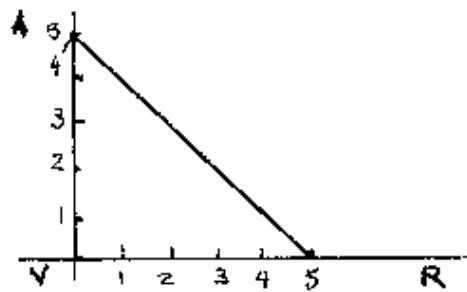


figure 1

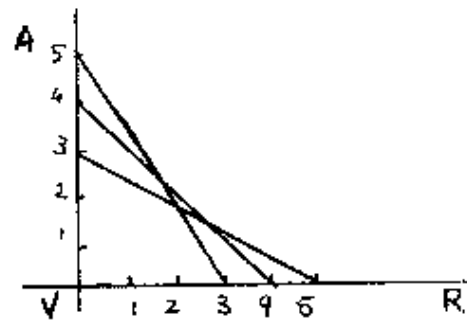


figure 2



figure 3

The amount of knowledge that is obtained with a given combination of accuracy and relevance is indicated by the triangle AVR, which area is given by  $AR/2$ . It is obvious that this area is bigger when  $A = R$ .

The conclusion we take from our exercise is that, if our presumptions have something to do with reality, it is not inconsequential which combination of accuracy and relevance is used at a given moment in the development of a science. An extreme concern with accuracy and objectivity leads to sterile search of universal laws, excessive precision on accounting and classification of irrelevant things, and so on. The sterility of an excessive concern with social or "normative" relevance consists in the disregard for

the procedures of verification, confirmation and consistency. The consequence is a "science" that does not have an internal structure, which is the result of following up all the logical and empirical implications of the first assumptions and findings. A "science" without this internal structure would be a science without specifically scientific problems, but only "normative" ones, that would not be handled in a systematic way. A position of equilibrium, which would maximize the area of our triangle, could lead, it seems, to "more knowledge", and perhaps to breakthroughs that could rise the absolute level of K. Thus, the alternative between accuracy and relevance will be progressively less heartbreaking. In short, *virtus in medium*.

## V

But is the difference between the two orientations we are discussing a simple question of emphasis? Isn't the concept of "verstehen" a radical departure from the canons of falsification, intersubjectivity and verification?

It is possible to say that there are at least two different kinds of problems involved here. The first is a substantive question, regarding a specific kind of phenomena: are the "cultural objects" in sociology, or the "conscious objects" in psychology worth knowing? Are they relevant as subjects to be explained, or as independent variables to the explanation of other phenomena?

The other question is of an ontological type, and consists in asking whether this kind of "soft" phenomena can be studied in an objective form, or are part of a region of transcendental facts that do not yield themselves to the analyst as phenomena. A fourfold table give us the possible combinations and types of answers to these questions.

are "cultural objects" subject to empirical knowledge?		are "cultural objects" relevant?	
		no	yes
	no	(Watson)	phenomenology, verstehen,
	yes	historical materialism	modern sociology and psychology

No doubt that these two questions usually occur simultaneously, and are a function of a low level of "K", in our previous discussion - or simply of the impossibility of actually handling in an effective way these kinds of phenomena.

An extreme behaviorism assumes, in fact, that "cultural" or "psychic" facts are not subject to empirical handling - - but, on the same token, excludes them from the range of things worth knowing. A "comprehensive" orientation, starting from the same belief, will hold that it simply indicates that empirical science is useless, and will try to develop techniques for reaching these facts in a "direct" form.

Marxists, mainly of the "historical materialism" type, have a philosophical stand against the existence of transcendental reality and are unable, therefore, to take either of these solutions. The way

Marxism comes to terms with the problem is, first, considering that, as "super-structures", facts of culture or consciousness are actually irrelevant - but there is nothing mysterious about them. And the theory of consciousness as a "reflection" from reality seems to be enough to give congruence to the argument<sup>25</sup>.

In spite of the philosophical commitments to the different answers (the "mythologie" referred to by Granger), social sciences developed, in fact, towards the empirical handling of phenomena considered before either irrelevant or unobservable. There is nothing more usual today than measurements of personality, studies of system and change of value orientations, game and decision-making theories, and so on. From this light the debate on the possibility of objective knowledge of subjective phenomena seems indeed old-fashioned<sup>26</sup>.

There is little left, apparently, from the "comprehensive" approach. All scientific knowledge depends, ultimately, on a selection of problems and aspects of reality, according to preferences and values. This leads to findings and presumptions that are afterwards developed and tested on their consistency and consequences. In this sense there is nothing special with the social sciences. And there is nothing special, either, with the nature of the object to be studied, as we have indicated above.

## VI

But the fact remains that sociology, in contrast with the natural sciences, is unable to account for the actual behavior of empirical phenomena in a precise and systematic way. The maximum it can do, and not very often, is the prediction of certain general types of outcomes within a given range or probability, which is not always specified.

E. Nagel offers an answer to the question of why social sciences have to rely so much on statistical statements, of a probability type, as against the general and precise laws developed by other disciplines. His argument is summarized below<sup>27</sup>.

The constitution of a science based on deductive explanation, as against probabilistic explanations based on statistical generalizations, implies the possibility of handling, conceptually as well as empirically, some basic "homogeneous items" that could yield stable outcomes and relationships. The way of getting to these homogeneous items is, it seems, by successive distinctions and discriminations from the raw material. But social sciences, for practical reasons, is not interested on cutting its object that far, and, so, is doomed to remain only with statistical generalizations.

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<sup>25</sup>Since Marxism is not interested in "subjective" problems, this rough concept of consciousness is never subject to conditions of refutability. But, at the same time, extensive resources are employed to prove that it is correct, since the theory relies so much on it. So the paradox of an intensive empirical, logical and philosophical analysis of a concept that remains, in spite of that, always unchallenged by the analysis.

<sup>26</sup>For a discussion of this point, coincident with ours, cf. Diesing, p., "Objectivism and Subjectivism in the Social Sciences", *Philosophy of Science*, 1966, 33, 1-2 (March-June), p.124

<sup>27</sup>Nagel, E., *Structure of Science*, Princeton: Princeton Univ. Press, 1961, specially pages 504-507.

It is remarkable how Nagel shares with Weber the idea that the only way of getting a general and deductive social science is through a reductionist technique - the difference being that, while Weber assumed that it would be necessary to get down to the psychological level, Nagel suggests that it would be necessary to go still deeper, to the level of biological mechanisms that are at the basis of the psychological processes. But none of them really accepts or suggests this reduction: it is just a kind of argument *ad absurdum*.

They seem to agree, also, that these "homogeneous items" exist, and could be reached if only this was the object of the inquiry<sup>28</sup> But the concept of "homogeneous items", even with the qualification that Nagel introduces ("in certain indicated aspects") seems to imply a kind of ontology that is clearly misplaced. No doubt that science works with concepts that tend to be analytically precise and homogeneous, but the way of getting at them is not by empirical dissection of the object, but by a specific kind of interplay between concept and empirical constructs.

To see this better, let us consider three kinds of theory building, in economics, chemistry and sociology. More specifically, let us consider the formula of a chemical reaction, the model of an economic process and the model of development of a given society.

The chemical formula is the outcome of an interplay between data and theory that is not a mere conceptual interaction, but a very concrete one. The objects of reality are manipulated according to some concepts, the concepts are reshaped according to the kind of manipulations the material allows to, or how it reacts to them, etc. At the end of this process, the data that corresponds to the theory, as expressed by the formula, is not a part of "reality" that is explained, but an empirical construct that is developed together with the conceptual construct that is its formula.

The same kind of concrete interplay cannot occur when the material is not subjected to this kind of manipulation. An economic model, referred to the behavior of a given actor in a market of a given type, does not predict, actually, the behavior of the real actors. It is, rather, a kind of Cartesian operation: reality is shaped at a conceptual level, through the analysis that reveal conceptually "clear and distinct" aspects of things, and these clear and distinct ideas are ordered in a clear and distinct way. This kind of model is heuristic, helps to understand reality, but does not reproduce nor shape it, and is unable, thus, to yield predictions.

A macro-economic model of the Keynesian kind is something different. This model is developed in societies that have achieved high levels of economic integration, and the economic activity is expressed by data that are generated by the same economic agents as determinants of their behavior - rates of interest, levels of employment, wages, profit, and so on. Thus the "happy fortune" of macro-economics, as suggested by Prof. Smelser - they deal with the very data that are produced by the empirical reality, the conceptual construct being a prolongation from the empirical construct that is an integrated market economy. This kind of economics can not only predict but also prescribe policies,

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<sup>28</sup>A similar kind of realism is found in Durkheim's search for "le cas pur", in *Les Formes Elementaires de la Vie Religieuse*. For a discussion, cf. Galtung, J., *Theory and Methods of Social Research*, Universitetet i Oslo, 1965 (Chapter 1).

which are, in fact, manipulations of the same variables that are produced and quantified by the empirical world and used by the theoretical model.<sup>29</sup>

Sociology, in contrast, does not have the benefit of these ready made data. nor is it allowed to construct its own object. Its relations with data are Platonic, in the sense that no actual intercourse is really possible. In this situation, the alternatives are limited. One consists in making a theoretical model based on axiomatic assumptions, without empirical references. The other would be to accept reality as it is, with all its imperfections, and take the measures that are possible in this condition. The models that sociology could construct, in this second alternative, would be statistical models, based on imperfect probability statements.

## VII

We are coming to the end of our argument. Before closing it, let us recapitulate.

We started by contrasting two perspectives regarding sociology, or the social sciences. One, given by Merton, states that sociology is a young science, and, in consequence, has to restrict itself to middle-range theorizing and empirical research, so that accumulation is achieved and a grand theoretical synthesis may come about. The other, taken from Weber, agrees that sociology is young, but adds that this is the natural and fortunate characteristics of a science of society.

These two points of view have different and relevant consequences regarding the characteristics of sociological inquiry, and we tried to spell them out.

Regarding the point of view represented by Merton, we argued, first of all that, social science, or sociology, did not seem to approach a mature stage in its evolution since Weber. Many developments occurred, but the conceptual and theoretical dispersion seems to be more intense than ever. We intended to show afterwards, how the scientific standards suggested by Merton are not derived from an analysis of sociology itself, but rather from an image or other disciplines. Discussing the shoulders of giant's aphorism we suggested that perhaps this is not the time - if the time will ever come - to forget the founders of social science.

Our discussion of Weber intended to indicate which points make social sciences such an a distinctive discipline, from his standpoint. The points are, first, that social science deals with historical data, and second that these data are meaningful. The next step consisted on showing how these two orientations come from different social contexts, where the roles performed by the social scientist are very different from each other, and, besides, from different, although contaminated, cultural traditions. But since this digression on sociology of knowledge does not substitute the epistemological discussion,

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<sup>29</sup>The limitations of a Keynesian model appear when it is applied to an underdeveloped context, where there is no integrated market economy and the usual assumptions of economic behavior do not hold. The consequence is that the economic indicators, that exist, do not account for the full mechanisms of the economic process, and their manipulation, therefore, does not yield the expected outcomes. In other words, there are no "homogeneous items". The debate on "structuralism vs. monetarism" among the economists of the United Nations Economic Commission for Latin America is a manifestation of this difficulty.

we proceeded to see whether the assumption that meaningful data are radically different from other kind of data really holds.

Our conclusion is negative. We saw, first of all, that this radical differentiation comes from a difference in emphasis that can be explained on sociological grounds. That these different emphasis could, eventually, lead to the search for non-empirical forms of knowledge, but that this problem is irrelevant at the light of the modern achievements of social sciences. Finally, we argued that the reason why sociology is unable to predict or intervene effectively in the real world, having to rely so much on loose statistical procedures, is not because of any special characteristics of its subject matter, but simply because of a peculiar form of relationship between this discipline and a given section of reality that is closed to the development of empirical constructs.

What we did not discuss, however, was Weber's contention that the subject of social sciences is historical, and, consequently, social sciences are bounded to historical changes. Our suggestion, regarding this point, is that it is indeed the task of social sciences to come to grips with historical facts *qua* historical. We do not imply that this is the only task of social sciences, but simply that it is a very relevant one, a kind of concern that was central to Max Weber and that cannot be simply left out in behalf of an ideal of general theorizing.

The fact is that the orientation sociology took in the last decades, mainly in the United States, towards the search for general - but limited - statements, is a combination of a given conception of science, the social limitations of which kinds of phenomena are subject to inquiry, and the actual limitations of conceptual and methodological tools for the analysis of more complex - and more historical - phenomena. The consequence was that the type of social sciences thought and developed by Weber became a quasi-illegitimate kind of intellectual endeavor, being driven apart from what could be called "the sociological establishment". Sociology, in the meantime, protected by the university system, became a skillful activity related to very general, but particular problems that only indirectly had some bearing on the external world or on the development of a true general theory. The paradox of a situation where general theory did not exist, but at the same time the founders began to be forgotten, was that sociology acquired the external characteristics of a "normal" science, dedicated, as Kuhn puts it, to puzzle-solving, without having the internal conditions of integration and consistency.

It is impossible for us to ascertain whether this was mainly a consequence of the internal difficulties of the discipline, or rather from the characteristics it took as a social system, part of the general academic system in the United States. Nor could we judge whether this development was really a handicap for the development of sociology. Our feeling is that this situation provided, actually, the conditions for the development of new approaches, new technologies, new conceptual schemes that allow us, today, to expect that the kinds of problems that concerned Weber can be approached with much more efficacy. But this only with the condition that there are changes in the three elements that used to characterize and condition the development of sociology. And we believe that some changes exist.

There are many indications that the role of the social scientist is changing drastically in the last few years, both in the United States and in Europe. In a society such as the American, where intellectual capacity is substituting for capital as the basic scarce good, the role of the University tends to transcend



the limits of a mere provider of skillful labor to the economic system. This, together with other factors, brings a climate of intellectual and political fermentation that makes it progressively unbearable to the social scientist to restrict his activity to the pursuit of general - but middle-range - findings. He feels that he is part of this change, and is entitled to be in the first front of the analysis and understanding of what is happening. This revolution in the role of the university is parallel to other revolutions - in race problems, in sex, in the international sphere. In this context the social scientist, less bound to academic isolation, is pressed to give answers, and also to give advice both in internal and external affairs. This kind of advisory function is another expression of the widening of his role, and brings changes in problems and perspectives<sup>30</sup>.

The role of the social scientist in continental Europe is also changing. The European "economic miracle" meant a reduction of the ideological and "intellectual" role of the social scientist, both of the Marxist and of the liberal types, or at least forced them to cohabit with the exigencies of a more stable and demanding academic milieu. This set the grounds for the import of the American type of sociology: European social sciences are becoming more empirical, less militant, and more oriented towards the academic community. But the more general, historical and speculative type of concern is not abandoned, in part because of intellectual tradition and in part because of the same factors that are changing the academic frames of American sociology. European and American social scientists, coming from opposite extremes, are now confronted with the same kind of problems. It would not be difficult to show how a similar concern exists among the social scientists in the socialist countries, where the ideological thaw is allowing the development of a differentiated social science, or in the countries of the "third world", where the impetus of "national liberation" is fading away and modern types of university are being created. It is unlikely that this process is leading to the creation of a sociological professional community integrated at the international level<sup>31</sup>, but at least a high level of international convergence of concerns, and contacts, is occurring.

Are these changes in roles leading to, or at least related to the emergence of a new concept of social sciences? The least we can say is that a new debate - or a new crisis - is on the way. At the time when continental Europe starts importing American sociology, it is curious to observe a tendency, in the United States, to revive the intuitionist and historical kinds of approach that had come to exhaustion in Europe, and this for the simple reason that American sociology is passing through the same problems that gave rise to these approaches<sup>32</sup>. The crisis that is going on (and which was, as a matter of fact,

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<sup>30</sup>The "Camelot" affair is a good example of the kind of crisis that occur when sociologists, previously restricted to their academic milieu, get involved with a type of activity that is loaded with unavoidable political repercussions. The reactions to the affair, ranging from surprise to the refusal to acknowledge the political aspects of the problem, indicate the lack of readiness for a more direct contact with the external world. Cf. Horowitz, I. L. (ed), *The Rise and Fall of Project Camelot*, The M.I.T. Press, 1967.

<sup>31</sup>The hopes that new forms of international contacts and institutions lead to the development of international professional communities, similar to the national ones, do not seem to be warranted by the facts. Cf., for instance, Eide Galtung, Ingrid, "Are International Civil Servants International?", *Proceedings of the International Peace Research Association Inaugural Conference*, Netherlands: Van Gorcum / Assen, 1966, p.198-209.

<sup>32</sup>It is remarkable how a concern with social change leads, very often, to the adoption of the most scientifically and philosophically conservative approaches that refuse to deal with reality in an empirical and systematic way. This

always latent) refers to the kind of questions we are discussing here: the appropriateness of Merton's aphorisms, the possibility and convenience of analyzing historical processes, of developing applied capability . . .

The precise conceptual status of the perspective that will emerge from this crisis is far from being settled, and it is beyond our possibilities - and perhaps illegitimate - to try to foresee it. It is very likely that we will not have *one* new conception and *one* new paradigm, but a plurality of simultaneous, complementary and, very often, incompatible lines of research and theoretical development. In general, however, we can expect that sociology, that has already given up the ideal of historical explanation, will also give up the ideal of general theory. We can expect that a new concept of middle range will emerge, not in the sense of second bests for grand theories, but rather in the sense of theories, or models, that refer to historical phenomena that have a limited level of generality.

Another paper, and extensive research, would be necessary to account for the conceptual and technological developments that are taking place in the social sciences and that makes them able to cope with the new demands, and give rise to new conceptions. For one thing, there are today better data, accumulation of research, retrieval systems and the computer. Theories of national and international development are derived from the characteristics of the international system, with the help of systematic statistical induction<sup>33</sup>; game and decision-making theories are used for the prediction of short-range outcomes<sup>34</sup>; international typologies are developed, and computerized models of society are tried<sup>35</sup>. These new procedures reintroduce the state as a relevant and valid unit of analysis, and the models and generalizations that are obtained aim to a proper middle-range level of generalization, and progressively strong possibilities of prediction. And since the world is changing, the systematic pursuit of new configurations of societal variables, that will shape the new world to come, are also becoming part of systematic research.

We referred above only to a few instances of new developments in an area that has a more direct bearing on the problems of historical change and macro-analysis, and it is obvious that many others, perhaps more decisive approaches, are also occurring simultaneously. In any instance, we believe that the new changes in sociology will never make it a "normal" and "mature" discipline where the constant

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alliance between social reformism and intellectual conservatism is well known in Latin America and Europe, and is just now coming to being in the American context.

<sup>33</sup>Cf Karl W. Deutsch, "The Theoretical Basis of Data Programs", in Merrit and Rokkan (ed), *Comparing Nations*, New Haven and London, Yale Univ. Press, 1966) as well as other works around this line of research.

<sup>34</sup>These strategical approaches can be taken as accounting for the subjective meaning of action, as suggested before. They are different, although not incompatible, with the study of general processes and trends that disregard the "internal motivations" of the actors' behavior. This distinction is stressed by A. Rapoport, "Two views on conflict: the cataclysmic and the strategic models", *Proceedings of the International Peace Research Association Inaugural Conference*, Netherlands, Van Groenou / Assen, 1966, p.78 - 99.

<sup>35</sup>The main reference seems to be Harold Guetzkow, *Simulation in International Relations*, New Jersey, Englewood Cliffs, Prentice Hall, 1963. For a Latin American attempt, cf. Cornblit, O., Di Tella, T., and Gallo, E., *Politics in the New Nations - A model of social change for Latin America*, Instituto Torcuato Di Tella, Buenos Aires, 1966.

concern with the basic scientific trends, and their social meaning, would be out of place, as it is said to be the case with the natural sciences. Because sociology has to develop according to a complex pattern of interplay between historical changes of meanings, values and social configurations; because the type of relations between the social scientist and his object is a direct function of the discipline as a social system; finally, because each new situation brings new specifically scientific problems, we agree with Weber that sociology was, after all, been granted the gift of eternal youth.